MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Professor H. H. TURNER, D.Sc., F.R.S., PRESIDENT, in the Chair.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Rafel Patxot Jubert, Passeig Bonanova 64, Barcelona, Spain (proposed by J. Comas Solá) ;

Ole Theodor Olsen, F.L.S., F.R.G.S., 116 St. Andrew's Terrace, Grimsby (proposed by Edward Roberts) ;

Captain Ernest William Owens, Local Marine Board, Dock Street, E. (proposed by Count de Miremont) ; and

Captain B. T. Stevens, 44 Bennett Park, Blackheath, S.E. (proposed by Thomas Lewis).

One hundred and one presents were announced as having been received since the last meeting, including, amongst others :—

M. Karl Bohlin, Sur le développement des perturbations planétaires, presented by the author ; Miss A. M. Clerke, Problems in Astrophysics, presented by the author ; Galileo, Opere, Edizione Nazionale, vol. viii., presented by the Italian

Government; A. Pannekoek, Untersuchungen über den Lichtwechsel Algols, presented by the author; Sydney A. Roberts, Azimuths of the north pole star, latitude 38° to 55° N., for use until the year 1915, presented by the author; Société Belge d'Astronomie, Bulletin, vols. i.–viii., presented by the Society; H. J. Zwiers, Recherches sur l'orbite de la Comète périodique de Holmes, presented by the Leiden Observatory

Drawing of curves of sun-spots and magnetic elements, 1841–96, presented by William Ellis; Drawings of sun-spots, 1850–1865, by J. H. Griesbach, presented by Henry Eichbaum.

Photographs of Comet *Perrine* &c., presented by V. Nielsen; Photographs of the Moon and Nebulæ taken by G. W. Ritchey with the 40-inch refractor and the 24-inch reflector (lantern slides and transparencies), presented by the Yerkes Observatory.

Letter from Mr. S. C. Chandler proposing the Establishment of a Southern Belt of Latitude Stations.

To the Royal Astronomical Society.

16 *Craigie Street, Cambridge, Mass.*

I beg to submit a statement with regard to a new problem that has arisen above the astronomical horizon within the past year, and to suggest, as a practical means for its possible solution, a proposition which it seems peculiarly fitting that the Society should take into consideration, for reasons that will presently appear

I will limit the present statement of the problem in question to its general features. Its history is very short. In the course of the researches for the establishment by observation of the relative motions of the Earth's axes of figure and rotation, commonly known as the variation of latitudes, there has emerged to view quite unexpectedly an entirely distinct phenomenon. The range of its effect is small, but the evidence of its existence is too definite to permit us to assume that it is not real without the most searching examination. Whether subjective or objective, its origin must be found before we can confidently complete the analysis of the motion of the poles with which it appears entangled in the observations. Its existence is the ingenious discovery of Mr. Kimura, of the International Latitude Station at Mizusawa, Japan, and was announced by him in No. 517 of the *Astron. Journal* (1902 Feb. 14), and in No. 3783 of the *Astron. Nachrichten* (1902 April 5). The only other literature pertaining to it will be found in *A. J.*, 524, and *A. N.*, 3808, and more recently in *A. J.*, 530. All the known facts with regard to it may there be readily examined, so that it is not necessary here to go into particulars. A sufficient description of

it is, that it is a small residual phenomenon with a total range of 0″·08, independent of the longitude of the meridian in which it is observed, but regularly variable with the time of the year, its maximum and minimum values being attained about January 1 and July 1. It is too pronounced and regular in its course to be ascribed to fortuitous arrangement of residuals. Consequently, whether it arises from some systematic error of observation or from some unknown but real cause, its explanation must be found.

In the paper last quoted I have endeavoured to examine two questions : first, whether it may be attributed to the effect of stellar parallax ; secondly, whether it may be due to an annual vibration of the Earth's centroid in the line of the terrestrial axis. I have shown that the first explanation would lead to what appear, according to our present notions, to be improbable values of the average parallaxes of the stars employed ; and that the second explanation requires a motion in the centroid of an amount that I suppose would be regarded as equally unlikely. I can imagine no other cause that will intelligibly account for the observed effect. Unknown anomalies in refraction may be responsible, but it is not easy to formulate the action of such an agency. Finally we have the possibility that the existing scheme of observation in the international belt of six stations in latitude $+39°$ is defective. Our only resource for further investigation of this subject, which I suppose must be attempted unless we propose to stop short in face of the dilemma, is to enlarge and vary the present programme of operations. In the paper mentioned I have suggested the establishment of at least one additional belt, to be located in the southern hemisphere ; and that two observers be provided for each station, so that the observations, instead of being confined to a few hours of each night, as in the present belt, shall embrace the whole diurnal arc between sunset and sunrise. It would be premature here to go into the details of arrangement, that being a matter for subsequent consideration, after it is decided that means for carrying out the project can be provided.

Now, the fact to which I desire to direct the attention of the Society is that there already exist observatories in the British Colonies, namely, at the Cape of Good Hope and at Sydney, N.S.W., admirably situated to serve as two stations of such a belt, so that if a third could be established about thirty miles south of Santiago de Chile, we should have a system extremely well adapted, both in longitude and latitude, for the purpose of a determinate solution of the coordinates of polar motion by observation of a common set of stars in the known way ; and the duplication of the observers and prolongation of the observed arcs would with much likelihood provide the means for solving the new enigma of Kimura's phenomenon. At least there is apparently no other resource that presents more chances for its solution.

This system of stations would be :—

	Lat.	Long.	Difference. Lat	Long.
	° ′	°	° ′	° ′
Sydney ...	−33 52	−151·2		
			0 4	132 7
Cape of Good Hope	−33 56	− 18·5		
			0 2	89 2
Santiago (30 m. S.)	−33 54	+ 70·7		

There are other grounds which make the establishment of such a belt in the southern hemisphere desirable. The phases of the annual term of the polar motion occur there in opposite climatic conditions to those in the northern belt.

The fact that two English colonial observatories are so fortunately situated, on the same parallel and in such advantageous difference in longitude, seems to make it appropriate that the Society should deliberately consider this proposition and, if it commends itself to them, lend their influence and encouragement to the undertaking of it under national auspices.

S. C. Chandler.

The foregoing letter was considered at the meeting of the Council of the Royal Astronomical Society (1903 March 13), and the following resolution was passed :—

That the Council of the Royal Astronomical Society is much impressed with the importance of establishing a Southern Belt of Latitude stations for two years at Sydney, the Cape of Good Hope, and near Santiago as proposed by Mr. Chandler, to investigate the subject of Mr. Kimura's term in the latitude variation, and considers it very desirable that this proposal should be carried into effect.

Velocity in the Line of Sight. Selected Stars. Cambridge Observatory, I. 1902. By H. F. Newall.

The present note is a first contribution to the plan of co-operation recently inaugurated under the initiative of Professor E. B. Frost, of the Yerkes Observatory (*Astrophysical Journal*, 1902 October, xvi. 169), and contains (i) a brief statement of the instruments used at Cambridge ; (ii) notes as to the measurement of photographs and the reduction of results ; (iii) a few results relating to the velocity in the line of sight of three out of the ten stars selected for the purposes of co-operative observations as velocity-reference-stars.

If apology is needed for the presentation of such meagre results, it may be offered on the ground that it seems desirable

to publish results for a given year as early as convenient after the end of the year ; and though the year 1902 was already well advanced before the plan of co-operation was really inaugurated, there is some advantage in publishing even meagre results at once, in the hope that the collation of the first contributions of the co-operating observatories will lead to the evolution of a form of publication best suited for the purpose in time for the more complete results of 1903.

The following notes summarise a description of the instruments used :—

Instruments :—

Equatorial : Aperture 25 inches (630 mm.), focal length 29 feet (8844 mm.). Used with correcting lens which changes the angular aperture of the convergent beam from $\frac{1}{14}$ to $\frac{1}{10}$, and effectively reduces focal length to 21 feet.

Spectrograph: Slit, reflecting speculum jaws after device of Huggins.

Collimator, aperture $2\frac{1}{8}$ inches (54 mm.), focal length $20\frac{1}{2}$ inches (520 mm.).

Prisms, four, flint, transmitting a 2-inch beam, deviation 180° for H_γ.

Camera, focal length 20 inches (508 mm.).

Temperature control, instrument encased in quilted cover ; no electrical heating at present (1903 January).

Measurement of Photographs.—Zeiss Comparator, one revolution of micrometer corresponds effectively to $\frac{1}{10}$ mm. Errors of scale have been carefully determined and are applied in all reductions.

The plates are measured in two positions : (i) red to right (R to *r*), (ii) red to left (R to *l*). In the position "red to right" the scale readings increase with increasing wave-lengths.

The scale of the photographed spectra and the values of a single turn of the micrometer screw are indicated in the following table :—

Wave-length.	Tenthmetres per millimetre.	Velocity $^{km}/_{sec}$ per turn of micrometer.
4200	12·2	87
4250	13·1	92
4300	13·9	97
4350	14·7	101
4400	15·6	106

Reductions.—Wave-lengths of stellar lines are determined with reference to comparison spectrum of Fe spark, by means of Cornu-Hartmann relation. Hence shift due to velocity &c. is found, $\delta\lambda = \lambda* - \lambda\odot$.

Tables and Standards.—Kayser's wave-lengths are used for comparison lines at present; but as the spark is used, special determinations will be necessary.

Rowland's tables for the solar spectrum are used for the values of $\lambda\odot$.

Schlesinger's tables for correction for Earth's orbital velocity (Sun's parallax, 8″·80), and a modification of Campbell's tables for correction for Earth's diurnal motion. Velocity of light, 300,000 $^{km}/_{sec.}$

Results.—The velocity given for each photograph referred to in the following table is the velocity of the star with reference to the Sun. It is expressed in kilometres per second, and is corrected for the orbital velocity of the Earth and for its diurnal motion, and also for the curvature of the spectrum lines, but otherwise is the simple arithmetic mean of the values determined for the various lines measured in the spectrum.

Star and No. of Plate.	Date and G.M.T.	Hour Angle.	Slit Width.	Range of Spectrum used.	Comparison Spectrum.	No of * lines.	Velocity reduced to Sun.
α Arietis. 1902	h m	h m	mm				km/sec.
F. 346	Sep. 16 12 17	21 51	0·025	4202 } 4405 }	Fe spark	17	−13·44
F. 353	Sep. 26 11 17	21 34	,,	,,	,,	17	−16·02
F. 374	Nov. 12 10 29	23 54	,,	,,	,,	17	−13·23
				Mean (3 photographs)			**−14·26**
α Persei. 1902	h m	h m	mm				
F. 349	Sep. 19 13 2	21 37	0·025	4250 } 4326 }	Fe spark	12	−3·14
F. 351	Sep. 24 12 54	21 49	,,	,,	,,	13	−1·89
F. 352	Sep. 25 11 55	20 53	,,	,,	,,	13	−1·83
F. 356	Sep. 30 11 27	20 47	,,	,,	,,	13	−6·24
F. 357	Oct. 2 10 40	20 6	,,			13	−1·48
F. 360	Oct. 15 8 52	19 9	,,			13	−0·57
F. 361	Oct. 15 9 52	20 9	,,			13	−2·32
F. 363	Oct. 18 10 32	21 1	,,			13	−4·72
F. 364	Oct. 22 9 42	20 27	,,			13	−1·22
F. 365	Oct. 22 10 52	21 37	,,			13	−2·96
F. 367	Nov. 8 9 37	21 29	,,			13	−1·86
F. 371	Nov. 12 7 45	19 51	,,			13	−2·68
F. 372	Nov. 12 8 35	20 41	,,			13	−4·37
F. 373	Nov. 12 9 14	21 20	,,			13	−1·31
				Mean (14 photographs)			**−2·61 ± 0·28**
				p.e. of single determination			± 1·04

Star and No. of Plate.	Date and G.M.T.	Hour Angle.	Slit Width.	Range of Spectrum used.	Comparison Spectrum.	No of * lines.	Velocity reduced to Sun.
α Bootis. 1902	h m	h m	mm	4202			km/sec.
F. 334	May 13 13 5	2 17	0·0084	4326	Fe spark	13	−4·81
F. 336	June 9 12 40	3 39	,,	,,	,,	14	−5·44
337	June 9 13 50	4 49	,,	,,	,,	14	−7·53
F. 338	June 17 11 0	2 27	,,	,,	,,	14	−4·87
F. 339	June 23 11 10	3 4	,,	,,	,,	7	−5·83
F. 341	July 9 11 11	4 8	,,	,,	,,	13	−6·31
				Mean (6 photographs)			**−5·80 ± 0·28**
				p.e. of single determination			± 0·68

Accuracy of Measurements.—In order to convey some idea as to the accuracy attained in the measurements, particulars of results of the reduction of plate F. 334 *α Bootis* are here given, with reference to twelve lines that were picked out in the comparison spectra of the iron spark and measured just as if they were to be used as standard lines, and also with reference to seventeen lines measured in the spectrum of the star. This plate was measured by myself * altogether four times, twice in each position of the plate, red to right (R to *r*) and red to left (R to *l*). The measurements of the iron lines and the star lines are dealt with separately, and by the term "completely independent sets of measures" it is intended to convey that (i) the plate was adjusted afresh each time on the micrometer for parallelism both of the spectrum to the slide and of the bisecting wire to comparison lines, and also for focus of the eyepiece, and (ii) the measurements were carried out on different days, so that all question of remembering appearances of individual peculiarities in the photographic images is eliminated.

A. *Comparison lines:*

(1) From two completely independent sets of measures, each R to *r*, differences were taken between the measures on each line.

The mean error of a single difference is $\pm 0^{mm}\cdot00089$.

Hence the mean error for a single line taken as the mean of two separate measures is $\pm 0^{mm}\cdot00045$.

Similar results are got from two completely independent sets of measures, each R to *l*.

(2) Next, from two independent sets of complete measures, each being the mean of a measure R to *r* and a measure

* My assistant, Mr. H. J. Bellamy, who has secured all the photographs from which the results recorded in this note have been got, has also measured the plates of α Bootis with results which are in good accord with my own; but I confine myself in the present note to my own measurements for the sake of uniformity, and propose to revert to a comparison of our results in another paper, which is to contain a description of the four-prism spectroscope.

R to *l*, differences were taken between the complete measures on each line.

The mean error of a single difference is $0^{mm}\cdot00079$.

The mean error for a single line taken as the mean of two measures R to *r* and R to *l* is $\pm0^{mm}\cdot00056$.

B. *Star lines:*

(3) From two completely independent sets of measures, each R to *r*,

The mean error for a single line taken as the mean of two measures, each R to *r*, is $\pm0^{mm}\cdot00075$.

(4) From two completely independent sets of measures, each R to *l*,

The mean error for a single line is $\pm0^{mm}\cdot00051$.

(5) From two independent sets of complete measures, each being the mean of a measure R to *r* and a measure R to *l*,

The mean error for a single line taken as the mean of two measures R to *r* and R to *l* is $\pm0^{mm}\cdot00058$.

Taking the extreme values of a turn of the micrometer in the range of spectrum measured as 87 $^{km}/_{sec}$ at the violet end and 99 $^{km}/_{sec}$ at the red end, we find from (2) and (5) that the probable error of a complete measure lies

for a single comparison line between $\pm0\cdot32$ and $\pm0\cdot37$ $^{km}/_{sec}$

and for a single star line between $\pm0\cdot33$ and $\pm0\cdot38$ $^{km}/_{sec}$.

The above results relate simply to settings of the micrometer wire on the lines, and are independent of any question of identification of lines. The peculiarities of what may be called right and left personality are eliminated.

Each of the four sets of measures of F. 334 has been reduced separately, and a velocity has been deduced for each of the 13 star lines used for reduction. Combining them in pairs so as to eliminate right and left personality, we get the probable error of a velocity determined from a single line as $\pm1\cdot3$ $^{km}/_{sec}$ for one pair of measures, and $\pm1\cdot4$ for the other, whilst the probable errors of the final means are $\pm0\cdot36$ and $\pm0\cdot38$ $^{km}/_{sec}$. The value obtained in this way is probably only serviceable as a measure of success in identification of the stellar lines.

If we take the mean velocity for each of the six photographs of *α Bootis* that have been measured, and give equal weight to each as a separate independent observation, we arrive at a more trustworthy value of the probable error of the velocity based on measurements of a single photograph, viz. $\pm0\cdot68$ $^{km}/_{sec}$.

Note on the Spectrum of α Bootis.—In the measurements of the spectra of *α Bootis*, eighteen lines were picked out for measurement in the range of spectrum dealt with, viz. $\lambda\ 4202-\lambda\ 4325$. On reduction four of these lines showed peculiarities and were omitted for the purposes of this note. They are evidence either

of peculiar conditions in the star or of the superposition of a second spectrum on the main one. Further measurements and observations have been planned with a view to elucidating the point.

On the Nebula h 2302 N.G.C. 7822 Cassiopeiæ ; the Region surrounding H *II. 457 N.G.C. 1665 Eridani, with ten new nebulæ ; and* H *III. 558 N.G.C. 7492 Aquarii.* By Isaac Roberts, D.Sc., F.R.S.

The nebula *h* 2302 is described by Sir John Herschel in the *Phil. Trans.* 1833 November, p. 481, as exceedingly faint, of a round figure, diam. 10′±.

Three photographs of the nebula and of the region surrounding it were taken with the 20-inch reflector and exposures of the plates during 90 minutes each, 1901 October 9, 1902 October 25, and 1902 December 2. They show the nebula to be a cloud of very faint nebulosity, irregular in light intensity and structure ; the stars DM. 1679, zone 66°, mag. 6·0, DM. 1676, zone 66°, mag. 7·8, and DM. 1675, zone 66°, mag. 9·0, which is the brightest in a cluster of bright and faint stars, are, apparently, involved in the nebulosity. The cloud measures about 42′ of arc from *preceding* to *following* and 38′ from *north* to *south* ; there are also faint indications, on the negative, of further extensions of the nebulosity which a longer exposure of the plate would reveal with greater density.

The region around the nebula H II. 457 *Eridani* N.G.C. 1665 includes many nebulæ, amongst which are the following prominent ones :—H II. 457 is shown on the photograph taken with the reflector 1903 February 17, to be a right-hand spiral viewed obliquely, nucleus stellar ; these features are not recorded by Herschel.

H III. 589 N.G.C. 1659. The photograph shows this nebula to be a spiral with bright stellar nucleus ; very faint star involved in the *north following* end ; these features are not recorded by Herschel.

H III. 588 N.G.C. 1643 is described by Herschel as extremely faint ; very small ; irregularly round ; brighter in the middle. The photograph shows it to be bright and pretty large.

h 330 N.G.C. 1656. Sir J. Herschel describes this nebula as extremely faint ; irregular figure ? The photograph shows it like a large stellar nucleus with extensions of faint nebulosity in *south following* to *north preceding* direction ; a faint star near it on the *n.f.* side.

N.G.C. 1645. D'Arrest describes it as very faint ; pretty small ; round. The photograph shows it like a pretty bright star surrounded by nebulosity.

N.G.C. 1666 and 1677 Swift V. are on the plate.

N.G.C. 1667 Stephan XIII. is probably a spiral nebula with small irregular nucleus, and large rather dense nebulosity, and not as described in the Catalogue. N.G.C. 1681 Stephan IX. is shown with a bright (not a faint) stellar nucleus surrounded by nebulosity.

Bigourdan 380 is shown on the photograph as a pretty bright stellar nucleus surrounded by nebulosity elongated in *s.f.* to *n.p.* direction.

The following ten nebulæ are shown on the photographic plate, but are not recorded in Dr. Dreyer's catalogues ; they are therefore assumed to be new. The approximate coordinates here given are for the epoch 1860, which is that of the Catalogue.

R.A. h m s	N.P.D. ° ′	
4 39 43	94 56·4	Stellar nucleus surrounded by faint nebulosity.
4 39 53	95 12·7	Small spiral nebula with bright stellar nucleus ; indication of star on south end.
4 41 31	95 36·6	Right-hand spiral nebula ; faint stellar nucleus.
4 41 51	95 22·9	Probably a spiral nebula ; faint and small ; elongated in *n.f.* to *s.p.* direction.
4 42 43	95 14·1	Small right-hand spiral nebula ; with stellar nucleus ; elongated ; indications of condensations.
4 43 27	95 19·9	Faint spiral nebula with faint nucleus ; elongated in *s.f.* to *n.p.* direction.
4 43 50	95 40·2	Faint spiral nebula, viewed obliquely, with faint stellar nucleus and condensations.
4 43 55	95 8·6	Nebula like small bright stellar nucleus surrounded by nebulosity.
4 44 51	96 28·2	Faint nebula, probably a spiral ; brighter in the middle ; elongated in *s.f.* to *n.p.* direction.
4 44 58	95 12·7	Very faint nebula with faint nucleus ; probably a spiral.

H III. 558 N.G.C. 7492 *Aquarii*, which was photographed with the 20-inch reflector and with an exposure of the plate during 90 minutes, 1902 December 18, is a cluster of very faint stars, described by W. Herschel, in the *Phil. Trans.* 1833, as a nebula eF ; vL ; 2 or 3′ ; the faintest thing imaginable.

Besides the nebulæ which have been described above, there are, on this as well as on nearly all of my negatives, such vast numbers of faint stars which are claimed by some astronomers to be nebulæ, that practically the name "Faint Star" might be altogether omitted from astronomical nomenclature. But at present I cannot admit any justification for such a stupendous departure from established usage. The irregular and nebulous appearance of the margins of the stellar images on the photographic plates are, doubtless, caused by atmospheric tremors during the exposures and by instrumental effects.

KEY MAP.

NEBULOSITIES IN ORION.

NEBULOSITIES IN ORION

PHOTOGRAPH BY DR. MAX WOLF, HEIDELBERG

On Three of Sir William Herschel's observed Nebulous Regions in Orion. By Dr. Max Wolf.

Dr. Roberts has recently published in the *Monthly Notices* (vol. lxiii. p. 26) an interesting paper upon Herschel's suspected nebulous regions. Photographing these regions systematically with his 20-inch reflector and 5-inch portrait lens, he has found that nearly all these places show absolutely no nebulosity. This seems to me strange, as both Professor Barnard and myself found the case to be the contrary. I had made contact prints from a photograph of the nebulosities connecting the great *θ Orion* nebula and the *ζ Orion* nebula. My photograph (Plate 11) covers three of Herschel's regions, which Roberts finds free from nebulosity, viz.:

Herschel's No.	R.A. 1900. h m s	Decl. 1900.	Herschel's Description.	Roberts' Description.
22	5 28 53	−6 56	Affected with milky nebulosity.	No nebulosity.
23	5 30 10	−2 43	Affected.	No nebulosity.
24	5 31 56	−4 18	Visible and unequally bright nebulosity. I am pretty sure that this joins to the great nebula in *Orion*.	No nebulosity.

The original plate was taken 1901 January 16 with the *a*-lens of the Bruce telescope of 16-inch aperture, exposure 6 hours 15 minutes.

Herschel's No. 22 : From the great *Orion* nebula in a south-western direction all is filled with whitish nebulosity, connecting the great *Orion* nebula with the neck and head of the great snake which lies round *Orion*, with *ζ Orionis* as centre. This snakelike nebulous wing was first photographed by Barnard and Pickering and afterwards independently by myself, using small portrait lenses. Herschel's No. 22 is in this connecting nebulosity. It is shown on half a dozen or more plates taken since 1891, and it can be seen very well on the plate given here.

Herschel's No. 23 lies east of *σ Orionis*. We see at once that there is bright nebulosity on the plate. We find between *ζ* and *ε Orionis* and the *θ Orion* nebula a beautiful weaving of nebulous masses, with rifts and channels and islands, the two great *Orion* nebulæ appearing only as concentrated clouds in this enormous nebulosity.

Herschel's No. 24 belongs to the same mass and consists of fine nebulous structures.

We find therefore relatively bright nebulæ in these three regions. They are not at all faint or diffuse, but are easily detected by photography, and show a fine network of the same kind as the great *Orion* nebula itself.

At the northern edge of the plate is that remarkable stream of nebulosity running nearly in a straight line southward from *ζ Orionis*. The curious embayment (*a* in the key map) is distinctly visible on the accompanying plate. Dr. Roberts' photograph does not show the whole of the southern part on account of the restricted size of his field. The linear nebulous arm is really curved farther south, going more eastward, and in this stream, in

$$\alpha = 5^h\ 37{\cdot}1^m \qquad \delta = -3^\circ\ 7'\ (1900{\cdot}0)$$

is a second bay (*b* in the key map) similar in form and size to the other.* This bay contains three fine stars free of nebulosity. The eastern neighbourhood shows several curved rifts in the nebulosity. It is impossible here to describe the whole in detail, but I would call attention to three places where the woven nebulosity is exceptionally beautiful :

	R.A. 1900.	Decl. 1900.
	h	° ′
(*a*)	5 33·6	−5 18
(*b*)	5 31·5	−6 43
(*c*)	5 33·7	−7 9

The concentration (*a*) forms a striking flat ring resembling a smoke ring. The concentration (*b*) is a pretty cloud somewhat similar to the "trifid" nebulæ. Its centre is the B.D. star −6°·1252 of the 9·3 magnitude. The third concentration (*c*), consisting of several bright clouds and dark channels, has much resemblance to the cloud following *ζ Orionis*. The fainter parts embrace the 5th magnitude star B.D. − 7°·1152 = *d Orionis*.

Königstuhl-Heidelberg Astrophysical Observatory :
1903 *February*

On the Period and Light Curve of (7514) *UY Cygni.*

R.A. = 20h 52m 16s, *Decl.* = + 30° 2′·8 (1900).

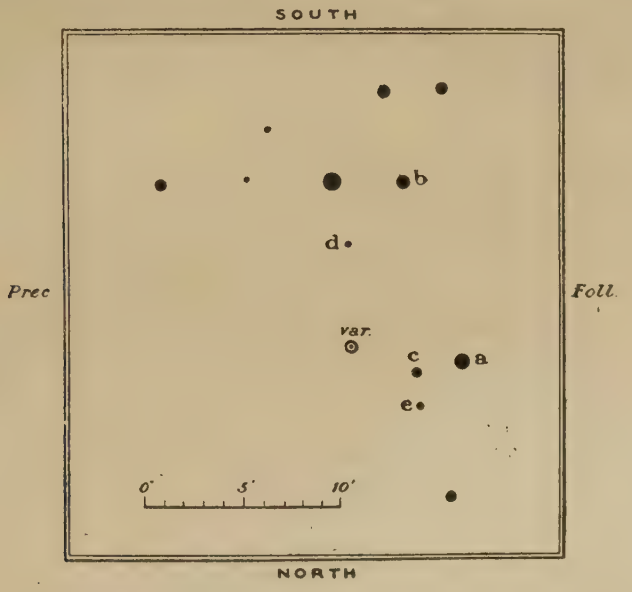
By A. Stanley Williams.

The observations on which the following results are based number altogether 238, and consist of 12 photographic observations from plates taken with a 4·4-inch portrait lens, 41 visual observations made with a 2¾-inch refractor, and 185 made with a 6½-inch reflector. But 15 of these 238 observations were rejected owing to their having been marked as uncertain, or as

* For the history of this nebula see the note by Miss Clerke to my article in the *Journal Br. Astr. Assoc.* 1890, p. 252.

possibly affected by cloud or fog ;* two on account of the time not having been recorded ; and six for discordance. One observation in addition was accidentally omitted, so that the actual number made use of is 214.

Most of the observations were made with the $6\frac{1}{2}$-inch reflector and a power of 73, and these have been considered as standard, and the other observations have been reduced so as to conform to them. It had at first been intended to use the reflector observations alone, at any rate for the purpose of constructing the light curve, but the others on reduction agreed so well with these on the whole, that they likewise have been employed. A few of the refractor observations were, however, given half weight.



The observations were generally made in the form of short sequences of steps, the sequence commencing usually with the comparison star *a* and ending with *d* or *e*. A light scale having been formed from about 50 such sequences observed with the reflector, all the observations, including those made with the refractor and the photographic observations, were as far as possible reduced to this scale.† The photographic observations

* Cloud and fog were abnormally prevalent in 1902, adding much to the difficulty of satisfactorily observing very rapid variables.

† The value of a step varies somewhat from night to night as regards the same group of stars, and even on the same night in the case of different groups. This is in conformity with the statement by Professor H. C. Wilson,

were reduced on the assumption that the photographic light changes are the same as the visual; an assumption which would not be correct if, for instance, the star underwent any decided change of colour in the course of its variations in brightness. As, however, the photographic observations when thus reduced agree satisfactorily with the visual ones, it is evident that the photographic variation is substantially the same as the visual, so far as regards this particular star. In making the visual observations the head was always so held that a line through the eyes was parallel to a line joining the two stars compared, in order to eliminate the effects of "position error."

The comparison stars used will readily be identified by means of the preceding little diagram, in which the brightest star shown is B.D. +29°·4240 (8·3 mag.). The brightness of each comparison star according to the adopted light scale is given below, together with the corresponding magnitude on the assumption that the magnitude of the star *c* is 10·20, and that the value of a step is 0·05 magnitude.

Comparison Stars.

Star.	Scale.	Magnitude.	Star.	Scale.	Magnitude.
a	36·4	8·81	*d*	0·2	10·62
b	26·1	9·33	*e*	0·0	10·63
c	8·6	10·20			

Owing to the unfavourable weather the number of completely observed maxima is not so large as had been desired, and in order to secure the best results it was found necessary to make use of a mean light curve. This provisional light curve was constructed in the following manner. The observations of the different maxima having been independently charted upon *transparent* squared paper, the results were superimposed one upon the other, and adjusted so as to correspond as closely as possible to what seemed to be the mean result. The superimposed sheets of squared paper were then held up to a bright light, and the mean curve carefully drawn. This resulting provisional light curve, it may be mentioned, is almost identical with the final light curve based upon all the observations, and reproduced in Plate 12. The exact times of maximum were then derived with the help of this provisional light curve from the separate observations of each night. The observed times of maximum are given in Table I. below. The weights in the last column were assigned at the time when the different maxima were determined.

when discussing the numerous observations of Nova *Persei*, to the effect that the value of a step was for most of the observers a variable quantity (*Popular Astronomy*, vol. ix. p. 481). In the case of the present star the observations were in most cases reduced so as to make the step value correspond to the mean, or standard, step value of the adopted scale.

TABLE I.

Observed Maxima.

No.	Ep.	Date. 1900.	Geoc. G.M.T. h m	Red. to ⊙ m	Helioc. G.M.T. h m	O−C. m	Wt.
1	0	Nov. 22	10 55	−0·4	10 54·6	− 1·0	1
		1902.					
2	729	Jan. 5	5 43	−4·8	[5 38·2]	[+86·3]	½
3	1038	June 27	11 22	+3·8	[11 25·8]	[+83·4]	...
4	1040	„ 28	12 28	+3·8	[12 31·8]	[−25·2]	...
5	1063	July 11	11 5	+5·1	[11 10·1]	[+44·2]	¼
6	1072	„ 16	11 40	+5·5	11 45·5	+13·2	¾
7	1138	Aug. 22	11 29	+6·5	11 35·5	− 1·6	¾
8	1145	„ 26	9 32	+6·5	9 38·5	−10·1	1
9	1147	„ 27	12 29	+6·5	12 35·5	− 7·6	1
10	1186	Sept. 18	9 34	+5·8	9 39·8	+ 9·9	1
11	1293	Nov. 17	9 13	+0·3	9 13·3	− 2·7	1

The first maximum, it should be mentioned, is photographic. The variable appears distinctly brighter upon this photograph than it does upon any of the others, and for this and other reasons it seems pretty certain that the star was either at, or at any rate very near, its maximum brightness at the time; whilst the long interval by which this observation precedes the others renders it of special value for determining the period of variation. The plate in question was exposed for one hour, from $10^h\ 25^m$ to $11^h\ 25^m$, on 1900 November 22; and a note states that the sky was rather hazy, particularly towards the end, when the altitude was lower. Under ordinary circumstances the maximum brightness of the variable would no doubt occur before the middle of the exposure, owing to the increase being more rapid than the decrease. But in the present case the greater haziness towards the end would tend to equalise matters, and the middle of the exposure has consequently been assumed as the actual time of maximum. The third and fourth maxima were observed by Dr. Hartwig at the Bamberg Observatory.* The bracketed maxima were not used in determining the period of variation.

From the data contained in the foregoing table the concluded elements of maximum are:—

Maximum = 1902 August 22, $11^h\ 37^{m\cdot}1$ G.M.T. + $13^h\ 27^m\ 2c^{s\cdot}85$ E.†

* See *Vierteljahrs. der Astron. Gesells.*, Jahrgang 37, Heft 3, p. 284.

† This period is $61^{s\cdot}26$ longer than that published by the writer in the *A. N.* 3771, and it would seem that in connecting the observations of the autumn-winter of 1901–02 with those of the previous year one period too many must have been assumed. This would account for the disagreement between the predicted and the observed times mentioned by Hartwig (*loc. cit.*).

and the residuals according to these elements will be found in the penultimate column of Table I. With reference to these, it may be mentioned that the maximum No. 2 is based only on observations made a considerable time after maximum, and in fact it rests practically upon a single observation ; whilst one of the two untimed observations, previously referred to, follows this one and shows that the star was probably estimated too bright at the first observation. The details of the two maxima observed by Hartwig are not at present known to me. In the case of maximum 5 the observations (five in number) are discordant *inter se*, and these five observations have consequently been rejected altogether, and they are included in the list of six observations rejected for discordance as already mentioned.

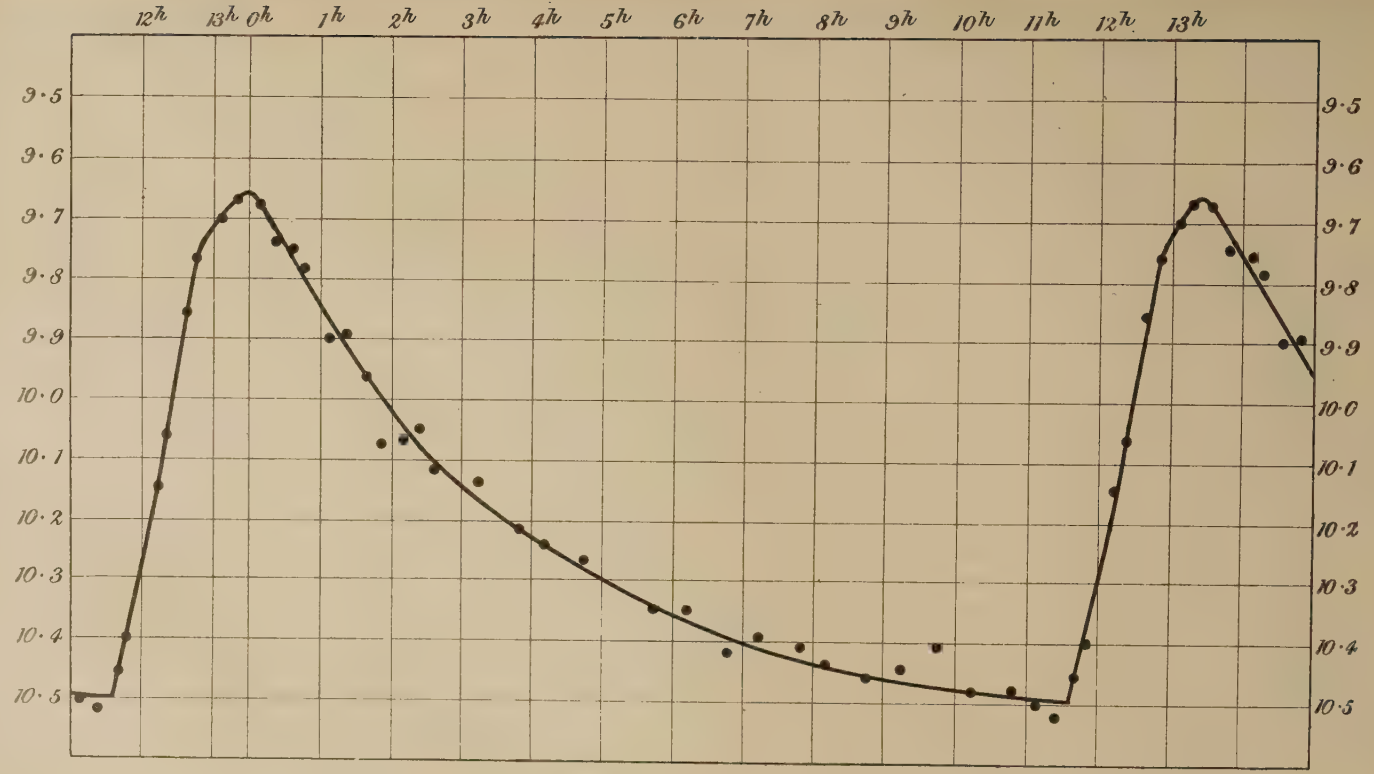
Owing to the great rapidity with which the rise from minimum to maximum takes place, the time when the variable attains to equality with the comparison star *c* can be observed with great exactness ; in fact with greater accuracy than can the actual maximum. This time is denoted by T_o, and Table II. contains the observed times of T_o and other data corresponding to Table I. It will be seen that the residuals are here very small ; so that similar observations in future years should enable the period to be ascertained with great exactness.

TABLE II.

Observed Times of T_o.

No.	Ep.	Date 1902	Geoc. G.M.T. h m	Red. to ⊙ m	Heliocc. G.M.T. h m	O−C m	Wt.
1	1138	Aug. 22	10 10	+6·5	10 16·5	− 0·1	1
2	1145	„ 26	8 23	+6·5	8 29·5	+ 1·4	1
3	1147	„ 27	11 18	+6·5	11 24·5	+ 1·9	1
4	1185	Sept. 18	8 27	+5·8	[8 32·8]	[+23·5]	¼
5	1293	Nov. 17	7 52	+0·3	7 52·3	− 3·2	1

The mean light curve has been derived by arranging all the observations in order of the interval by which they follow the last preceding maximum, and then forming them into groups, and taking the means. For about two hours before and after maximum the observations have been formed into 15-minute groups, and for the remainder of the period into half-hourly groups, with the exception of one group, which is an hourly one. The reduction to the Sun has been effected for all observations made when the star's brightness was changing rapidly. The resulting mean values were then plotted upon squared paper on a large scale, and a smooth curve drawn carefully through the dots representing the mean results. This final mean light curve is given on a smaller scale in Plate 12. The data upon which it is based are contained in Table III. Column 4 of this table gives the magnitude from the mean light curve, derived



LIGHT CURVE OF UY CYGNI.

from the large scale diagram referred to above; and column 5 the difference between the observed brightness and the brightness according to the curve. The mean difference, without regard to sign, is 0·019 magnitude; whilst the largest difference only amounts to 0·07 magnitude.

TABLE III.

Grouped Observations.

No.	Interval from prec. maximum h	m	Observed Magnitude	Magnitude from curve	O−C Mag.	No. of Obs.
1	0	8	9·67	9·68	−0·01	7
2	0	26	9·73	9·73	·00	5
3	0	39	9·75	9·77	− ·02	5
4	0	50	9·78	9 80	− ·02	3
5	1	7	9·90	9·87	+ ·03	7
6	1	23	9·89	9·92	− ·03	5
7	1	39	9·96	9·96	·00	7
8	1	51	10·07	10·00	+ ·07	6
9	2	8	10·06	10·04	+ ·02	8
10	2	22	10·04	10·08	− ·04	4
11	2	42	10·12	10·12	·00	6
12	3	15	10·13	10·17	− ·04	5
13	3	49	10·22	10·22	·00	8
14	4	14	10·25	10·24	+ ·01	7
15	4	44	10·27	10·28	− ·01	6
16	5	40	10·34	10·33	+ ·01	9
17	6	14	10·34	10·36	− ·02	6
18	6	47	10·42	10·39	+ ·03	4
19	7	15	10·39	10·41	− ·02	8
20	7	47	10·41	10·43	− ·02	5
21	8	13	10·43	10·44	− ·01	10
22	8	49	10·46	10·45	+ ·01	3
23	9	15	10·43	10·46	− ·03	7
24	9	43	10·41	10·47	− ·06	9
25	10	14	10·48	10·48	·00	6
26	10	45	10·48	10·49	− ·01	4
27	11	7	10·50	10·49	+ ·01	10
28	11	26	10·52	10·49	+ ·03	5
29	11	38	10·46	10·46	·00	6
30	11	51	10·40	10·33	+ ·07	5
31	12	11	10·14	10·14	·00	5

No.	Interval from prec. maximum. h m	Observed Magnitude.	Magnitude from curve.	O−C Mag.	No. of Obs.
32	12 22	10·06	10·04	+ ·02	4
33	12 37	9·85	9·89	− ·04	4
34	12 53	9·76	9·76	·00	5
35	13 8	9·70	9·70	·00	4
36	13 23	9·67	9·67	·00	6

It will be seen that the light curve is a somewhat peculiar one, being remarkable for the very rapid rise from minimum to maximum. The decline too is very quick at first, becoming gradually slower and slower, whilst the star remains with but little change near minimum brightness during nearly half its period. The curve is thus almost a facsimile of that of *Y Lyræ*.* Amongst isolated stars there seems to be only one other possessed of quite this extreme form of light curve and having a period of about half a day. This is *S Aræ*. The following is the description given by Dr. A. W. Roberts of the light changes undergone by this latter star. 'The remarkable feature about this star is its rapid rise to maximum. In 1^h 10^m it passes from its minimum to its maximum phase. The rate of increase is $0^m{\cdot}1$ every five minutes. After maximum passage the rate of variation steadily slows down till some three hours before minimum it is almost stationary.'† This description would apply equally well to either *UY Cygni* or *Y Lyræ*, with some little modifications arising from the slightly longer periods of these two stars. The resemblance is brought out very strikingly if the following elements of the three stars are placed parallel to each other.

	Y Lyræ	UY Cygni	S Aræ
Period	12^h 3^m 52^s	13^h 27^m 21^s	10^h 50^m 47^s
Duration of rise	1^h 40^m	1^h 53^m	1^h 10^m
Ratio increase to decrease	0·16	0·16	0·11
Range of variation ...	mag. 1·03	mag. 0·84	mag. 1·2

It is to this extreme type of variation combined with a period approximating to half a day that Professor S. I. Bailey seems to have applied the name of 'Cluster Type.'‡ But as regards the general *form* or characteristics of the light curve there would seem to be no essential difference between the above-mentioned three stars, or the stars of the so-called Cluster type, and the stars of the ordinary short period type, like δ *Cephei*, *T*

* See *Monthly Notices*, vol. lxii. p. 206.

† *Astronomical Journal*, xxi. 91.

‡ *Astrophysical Journal*, x. 258. Dr Hartwig has used the term 'Antalgol-type' to designate the extreme type of variation exhibited by *Y Lyræ* and *UY Cygni*.

Vulpeculæ, and others. The only material difference is that in the case of the average short period variable the ratio of the increase to the decrease averages about 0·34, as compared with 0·14, the average ratio in the case of the above-mentioned three stars, and that the period is also usually much longer. There is, however, no distinct line of demarcation between the two classes, and variables are to be met with in almost all the intermediate stages. Thus, the ratio of increase to decrease is 0·22 in the case of *V Velorum*; 0·23 in the case of *RV Scorpii*; 0·24 in the case of *U Triang. Austr.*; 0·27 for *V Centauri*; 0·29 for *T Moncerotis*; the same for *R Muscæ*; 0·30 for *ζ Cephei*; 0·31 for *T Vulpeculæ* and *Y Sagittarii*. Also this ratio is 0·14 in the case of *U Carinæ*, although the period of this star is as long as 38·7 days. The mean ratio for 36 short period variables (not including *U Carinæ*, *S Aræ*, *Y Lyræ* and *UY Cygni*) is 0·34.* It appears, therefore, that the so-called 'Cluster type' is really only an extreme form of the ordinary short period type.† Nevertheless, as it is convenient to have some suitable term to designate this extreme type of short period variation, it seems desirable to retain the name of Cluster-type, unless that of Antalgol-type should be preferred.

It has already been mentioned that six observations were rejected for discordance. Five of these, near the maximum of 1902 June 11, were rejected on account of their being discordant *inter se*. The remaining case of discordance is somewhat peculiar. The observation was made on 1901 December 19, with the $2\frac{3}{4}$-inch refractor at $1^h\ 4^m$ before the computed time of maximum, and according to this observation the star had already attained to nearly its full brightness. The cause of this may possibly be that this maximum occurred, or the rise set in, a little earlier than usual; particularly since, as in the case of *Y Lyræ*,‡ there is some evidence to show that the maxima are not all quite alike, some being more accentuated than others. It is difficult, however, to decide how far such differences may not be due to subjective causes; and in the case of an isolated observation it is impossible to be certain of the absence of fog or cloud, even though none was noticed at the time.

Table IV. contains the full elements of the variable, and Table V. the computed heliocentric Greenwich mean times of every tenth maximum during the remainder of the present year.

* These ratios have been derived from the data contained in Dr. Chandler's Third Catalogue of Variable Stars, and in Roberts' Catalogue of Southern Variable Stars in the *Astronomical Journal*, Nos. 491–492.

† I am indebted to Mr. P. S. Yendell, of Dorchester, Mass., U.S.A., for kindly drawing my attention to the similarity in the forms of the light curves of the 'Cluster type' and ordinary short period type of variation. Roberts has already remarked this in the case of *S Arae*.

‡ See *Monthly Notices*, lxii. 204.

TABLE IV.

Elements of UY Cygni.

Period of variation	day 0·560658 =	h m s 13 27 20·85	
Epoch of T_0	1902 Aug. 22, = J.D. 2415984·4282	10 16·6	G.M.T.
Epoch of maximum	1902 Aug. 22, = J.D. 2415984·4841	11 37·1	,,
Maximum brightness... ...	9·66 mag.		
Minimum brightness	10·50 mag.		
Minimum to maximum ..	$1^h\ 53^m$		
Maximum to minimum ...	$11^h\ 34^m$		
Ratio increase to decrease ...	0·16		

TABLE V.

Computed Times of Maximum.

Ep.	Date. 1903.	G.M.T. h m	Ep.	Date. 1903.	G.M.T. h m
1548	April 9	8 30	1788	Aug. 21	21 53
1558	14	23 3	1798	27	12 26
1568	20	13 37	1808	Sept. 2	3 0
1578	26	4 10	1818	7	17 33
1588	May 1	18 43	1828	13	8 7
1598	7	9 17	1838	18	22 40
1608	12	23 50	1848	24	13 14
1618	18	14 24	1858	30	3 47
1628	24	4 57	1868	Oct. 5	18 21
1638	29	19 31	1878	11	8 54
1648	June 4	10 4	1888	16	23 28
1658	10	0 38	1898	22	14 1
1668	15	15 11	1908	28	4 35
1678	21	5 45	1918	Nov. 2	19 8
1688	26	20 18	1928	8	9 42
1698	July 2	10 52	1938	14	0 15
1708	8	1 25	1948	19	14 49
1718	13	15 59	1958	25	5 22
1728	19	6 32	1968	30	19 55
1738	24	21 6	1978	Dec. 6	10 29
1748	30	11 39	1988	12	1 2
1758	Aug. 5	2 13	1998	17	15 36
1768	10	16 46	2008	23	6 9
1778	16	7 19	2018	28	20 43

Multiples of the Period.

	d h m		d h m
1p =	0 13 27·3	6p =	3 8 44·1
2 =	1 2 54·7	7 =	3 22 11·4
3 =	1 16 22·0	8 =	4 11 38·8
4 =	2 5 49·4	9 =	5 1 6·1
5 =	2 19 16·7	10 =	5 14 33·5

Hove: 1903 March 4.

Observations of Comet b 1902 (*Perrine*), *from Photographs taken at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The following table of Right Ascensions and Declinations of Comet *b* 1902 are, with the exception of those on September 6, 12, and 13, obtained from photographs with the 30-inch reflector. That on September 6 was taken with the astrographic equatorial, and those on September 12 and 13 with the 26-inch refractor. Owing to the rapid motion of the comet it was desirable to obtain photographs with shorter exposures than these instruments required, and accordingly from September 16 the reflector was used.

The number of exposures on each plate was in most cases four. The four images of the comet were measured in direct and reversed positions of the plate by Mr. Davidson and Mr. Melotte. Eight reference stars were measured, two of the images being measured direct and reversed by Mr. Davidson, and the other two by Mr. Melotte.

The catalogues of the *Astronomische Gesellschaft* were used for the positions of the reference stars, which were usually chosen so that their mean position should be near the centre of the plate. The stars were generally chosen between the limits 6 and 22 of the réseau, *i.e.* within 40′ from the central réseau lines, which are numbered 14.

The right ascensions and declinations of the comet given in the table are corrected for the part of the aberration which arises from the motion of the Earth, but not for the part arising from the movement of the comet itself. The corrections for parallax have not been applied.

Right Ascensions and Declinations of Comet b 1902 (*Perrine*), *from Photographs taken with the* 30-*inch Reflector and the* 26-*inch Refractor of the Thompson Equatorial and with the Astrographic Equatorial.*

Date. 1902.				R.A. 1902·0.			Decl. 1902·0.			Parallax. R.A. Log. Factor.	Parallax. R.A. Corr. s	Parallax. Decl. Log. Factor.	Parallax. Decl. Corr. s
	h	m	s	h	m	s	°	′	″				
Sept. 6	13	29	30	3	11	35·88	+37	2	49·6	9·4765	−0·27	0·4729	+ 2·7
12	11	36	27	2	59	18·09	40	25	48·0	9·6197	0·45	0·5354	3·7
13	10	53	45	2	56	25·23	41	5	30·7	9·6584	0·51	0·5883	4·4
16	8	50	18	2	45	35·70	43	18	27·9	9·7032	0·64	0·7224	6·7
16	9	29	9	2	45	28·54	43	19	48·4	9·7023	0·64	0 6656	5·9
17	9	35	33	2	40	47·28	44	10	59·3	9·7045	0·67	0·6339	5·6
18	8	37	5	2	35	44·19	45	2	31·4	9·7162	0·71	0·7054	6·9
18	9	16	58	2	35	34·93	45	4	5·0	9·7137	0·71	0·6427	6·0
19	9	59	2	2	29	25·37	46	2	33·5	9·6968	0·71	0·5324	4·9
20	9	31	33	2	22	44·48	47	1	15·3	9·7151	0·77	0·5547	5·3
21	8	45	56	2	15	14·28	48	1	57·3	9·7368	0·85	0·6102	6·3
22	9	20	27	2	6	7·98	49	8	59·7	9·7246	0·86	0·4933	5·1
25	7	38	0	1	31	21·36	52	31	8·0	9·7799	0·89	0·5789	7·1
26	9	9	2	1	14	30·58	53	44	7·5	9·7934	1·21	0·5825	+ 7·4
27	11	31	29	0	54	4·10	54	54	51·2	9·2122	0·33	9·6108	− 0·8
28	10	41	56	0	33	15·75	55	49	10·3	9·3755	0·51	9·5857	− 0·8
29	6	59	14	0	12	43·35	56	27	8·8	9·7984	1·40	0·3605	+ 5·1
Oct. 2	8	42	7	22	41	40·26	56	31	27·8	9·3425	−0·55	9·7385	− 1·4
8	7	9	47	19	54	42·23	42	24	22·5	8·6592	+0·12	0·1396	+ 3·6
10	10	31	37	19	17	20·40	34	46	49·3	9·6160	+1·05	0·6455	+11·3
12	6	57	44	18	53	4·64	28	24	54·7	9·1811	+0·36	0·5576	8·6
14	6	31	45	18	33	4·97	22	15	14·8	9·1682	0·33	0·6479	9·9
15	8	54	50	18	23	59·19	19	11	7·6	9·5327	0·72	0·7505	11·9
16	6	40	20	18	17	20·70	16	50	45·3	9·2840	0·39	0·7169	10·6
17	8	48	32	18	10	12·66	14	15	10·3	9·5360	0·67	0·7853	11·9
18	8	33	38	18	4	25·02	12	5	31·1	9·5275	0·63	0·7940	11·6
21	6	26	12	17	50	16·52	+ 6	40	31·8	9·3609	0·38	0·7742	9·8
26	6	37	25	17	32	27·57	− 0	9	24·8	9·4519	0·38	0·8388	9·3
29	6	20	54	17	24	3·16	− 3	12	46·0	9·4588	+0·35	0·8496	+ 8·6

Note on Photographs of Comet d 1902 (*Giacobini*) *taken with the 30-inch Reflector at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

Photographs of this comet have been obtained on 18 nights between January 15 and March 11. Till February 2 the telescope was guided on a star and the comet allowed to trail, but from February 16 the telescope was guided mechanically to allow for the motion of the comet.

The following list of photographs taken is supplementary to that in the *Monthly Notices*, vol. lxiii. No. 3.

Date. 1903.	Exp. m	Date. 1903.	Exp. m
January 15	10	February 23	10, 10
„	10	25	10
25	10, 7	26	15, 15
28	10, 10	28	15, 15
February 1	15, 15	March 1	10, 15
2	15	3	15, 15
16	20, 15	4	15, 5
17	15, 11	6	10, 10
18	10, 10	11	10, 10
21	10, 10		

Note on Photographs of Comet a 1903 (*Giacobini*) *taken with the 30-inch Reflector at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

Photographs of this comet have been obtained on 14 nights. The telescope was made to follow the stars so that the images of the comet are small trails.

The comet has increased in brightness very fast, and on March 11 had the appearance of a 4th magnitude star.

The following list gives the photographs taken to the present time.

Date. 1903.	Exp. m	Date. 1903.	Exp. m
January 28	10, 10	February 10	9
31	9	„	9
February 9	9	11	7
„	9	„	7

Date. 1903.	Exp. m	Date. 1903.	Exp. m
February 12	5	February 26	2, 2
17	7, 7	March 3	2, 2, 2
18	5, 5	6	2, 2
23	3, 4	11	$1\frac{1}{2}$, $1\frac{1}{2}$
25	2, 3		

On the Desirableness of a Re-investigation of the Problems growing out of the Mean Motion of the Moon. By Simon Newcomb.

Should the question be asked, What is to-day the most important unsolved problem growing out of the celestial motions? a survey of the field could, it seems to me, lead to but one reply. It is that of the discrepancies between the observed mean motion of the Moon and the most exhaustive investigations of the theory of that motion. Twenty-five years have now passed since the publication of my "Researches on the Motion of the Moon," * and during that time little of a comprehensive and definitive character has been added to the solution of the problem. A general review of it may therefore be appropriate at the present time. Such a review I now attempt.

I.

Beginning in a general way, we call to mind that, when slowly varying discrepancies are found between the predicted mean longitude of the Moon and that given by observation, two explanations are possible. One is that there is an actual deviation of the Moon from the theory on which the prediction is based; the other that the deviation is only an apparent one, due to a slow variation of the Earth's axial rotation, producing a corresponding change in our measure of time. Such being the case, the logical method of proceeding is to make an exhaustive investigation of the inequalities of long period in the mean motion of the Moon which may be produced by every known cause, to introduce any corrections which may be required to the theory, and then to attribute such deviations as remain outstanding to changes in the Earth's rotation. This is what I attempted in the work which I have quoted. The result was that the reconciliation of theory and observations in this way indicated changes in the rotation amounting in their accumulated effect to nearly half a minute of time in one direction or the other in the course of the last 250 years.

Such a result should, before being accepted as final, be tested

* *Washington Observations for* 1875, Appendix II.

by some independent method. Two conditions are available : the one based on the observed motion of *Mercury*, as determined by observations of its transits across the disc of the Sun ; the other on the motion of *Jupiter's* first satellite, as determined from eclipses. The first of these conditions is the more precise, and is the only one which any serious attempt has been made to apply. In my discussion of observed transits of *Mercury* up to 1881* I found that seeming variations of the Earth's rotation were indicated, in which there was a fair agreement in algebraic sign with those inferred from the motion of the Moon, but of a magnitude which, in the general average, was little more than one-third as great. Again, in a paper presented to the French Academy of Sciences in 1896 † I presented the results of a similar comparison brought up to the transit of 1894, based on the comparison of my new tables of *Mercury* and of the Sun. Only the November transits were used in this discussion, the May ones being of scarcely any weight. The results of this discussion and their comparison with those inferred from the motion of the Moon are summed up in the following table, where Δt is the amount by which the actual Earth must be supposed in advance of a uniformly rotating Earth. The dates 1690, 1741, and 1864 are the means for two transits ; the others are those of the individual transits. In the case of the Moon the results are interpolated from the table found on p. 266 of "Researches."

	Δt.		
Date.	Mercury.	Moon.	Diff.
	s s	s	s
1690	+15 ± 10	+30	+15
1723	+ 8 ± 5	+17	+ 9
1741	+ 2 ± 4	+ 5	+ 3
1769	+ 6 ± 5	−12	−18
1789	− 8 ± 3	−18	−10
1802	− 4 ± 3	−16	−12
1822	+ 4 ± 5	− 9	−13
1848	− 7 ± 3	0	+ 7
1864	− 5 ± 2	+ 5	+10
1881	+ 4 ± 2	+16	+12
1894	+ 2 ± 2	+17	+15

It will be seen that in the case of *Mercury* the corrections to the times as indicated by observations are greater than their probable errors in eight out of the twelve cases, and in three cases are more than twice the probable error. Although they give colour to the hypothesis of variability in the measure of time, both by their magnitude and their constancy of sign from

* *Astronomical Papers of the American Ephemeris*, vol. i. p. vi.

† *Comptes Rendus*, 1896, tom. I.

1690 to 1769, they are not large enough to prove it. But in any case they are too small to be compatible with the large deviations inferred from the motion of the Moon. We may interpret the column of differences as the outstanding residual errors in the observed times of transits of *Mercury* in case we accept the results from the Moon as real.

The evidence seems almost conclusive that the very improbable deviations in the Earth's rotation inferred from the observation of the Moon are unreal, and that the motion of our satellite is really affected by causes which have, up to the present time, eluded investigation.

II.

The next questions are, What has been done in the way of investigating the possibility of such inequalities on the theoretical side, and by what means may we hope to reach a definitive conclusion on the subject? I need not go over in detail the researches of La Place and Poisson, and need only briefly allude to the work of Hansen, these being sufficiently set forth in the works already quoted. It will suffice to say that Hansen announced two inequalities of long period, due to the action of *Venus*, which he introduces into his tables of the Moon, and which, according to him, completely reconciled theory with observation. But within twenty years of the publication of his tables observation showed a rapidly increasing deviation, and a discussion of observations of occultations before 1750 showed discrepancies of more than 30″ in the latter part of the seventeenth century. It also turned out that one of these inequalities had no foundation in theory, and that Hansen's coefficient was so assigned as to represent observations from 1750 to 1850. This inequality must therefore be thrown out altogether. Four completely independent determinations of the other inequality have been made. Using the notation

g, the mean anomaly of the Moon;
E, the mean longitude of the Earth;
V, the mean longitude of *Venus*;
$A = 18\,V - 16\,E - g$;

these results are the following. All are reduced to the mass $1 \div 408{,}000$ of *Venus*.

Delaunay *	...	...	$16'\cdot34 \sin (A + 29^\circ\cdot5)$
Hansen † ...	...	...	$15'\cdot34 \sin (A + 30^\circ\cdot2)$
Newcomb ‡	...	...	$14'\cdot80 \sin (A + 30^\circ\cdot5)$
Radau § ...	...	...	$14'\cdot14 \sin (A + 30^\circ\cdot0)$
Delaunay (cor.)	...	...	$14'\cdot12 \sin (A + 29^\circ\cdot5)$

* *Conn. des Temps*, 1862, addition. † *Tables de la Lune.*
‡ *Astronomical Papers of the American Ephemeris*, vol. v.
§ *Annales de l'Observatoire de Paris: Mémoires*, tom. xxi.

Delaunay's result was corrected by Tisserand, and later by Radau, who concluded that it should be diminished by 2″·2 in consequence of terms omitted in the development. The last of the preceding expressions shows this corrected value.

It is impossible to say, until after a thorough re-investigation of the whole subject on an independent basis, that any of these results should be accepted to the entire exclusion of all the others. A strong presumption in favour of the Delaunay-Radau value is weakened only by the fact that those investigators used substantially the same method of development. But the close agreement of all four, taken in connection with the fact that the methods by which the computations are made are different from the beginning, seems to leave no room for doubt as to the general accuracy of the result, provided that we leave out of consideration quantities of the second order arising from the mutual perturbations of *Venus* and the Earth.*

During the period in question two attempts have been made to discover, by an exhaustive investigation, whether any undiscovered inequalities, due to the action of any planet, can possibly subsist. One of these was made by the writer, the next by Radau. In the first no attempt was made to determine the actual numerical value of the terms investigated, but only to set an upper limit to their possible value. Radau's work is more exhaustive. He attempts to determine the actual numerical value of every inequality due to the action of the planets, but finds none of sufficient magnitude to explain or materially reduce the discrepancy.

The only known causes to which we can attribute changes in the Moon's mean motion are the action of the planets and the action arising from the deviation of the Earth from a spherical form. Both actions have, to all appearances, been exhaustively investigated. Where, then, are we to look for a solution of the enigma? The whole question must be re-investigated *ab initio* with a view to discovering whether any omission or error has been made, either in the theory or in the complex process of comparing it with observations. As a sort of forlorn hope, it seems advisable to determine whether by any possibility the mutual perturbations of the Earth and *Venus* can give rise to a sensible term in the Moon's mean longitude. Moreover, every

* In this connection a curious coincidence may be pointed out. In my "Researches" I showed that if we simply changed the sign of the constant term in the preceding expression, so that the argument should be $A - 30°$ instead of $A + 30°$, the agreement between theory and observation would be fairly good throughout the entire period of 250 years of observation. So striking a coincidence led a writer in the *Vierteljahrsschrift der Ast. Gesellschaft* to accept the conclusion that the change was real, and that there was an error of sign in the term in question as used by Hansen. But this conclusion seems quite out of the question; in fact, I investigated it thoroughly before the publication of my work. It would seem, therefore, that we can look upon the case only as a singular coincidence.

investigator who takes up the theory may well make a thorough inquiry into the question of the possibility of some periodic terms having been overlooked in the discussions so far made.

III.

What has been said in Section I. rests on comparisons of the observed mean longitude of the Moon at widely distant epochs with theory, the results of which are summed up on pages 264–266 of the "Researches" already alluded to. This comparison, though it seems fairly certain so far as general facts are concerned, may still need not unimportant corrections, especially during the period 1750–1840. It is remarkable that the history of the Moon's mean motion is more accurately known during the seventy-five years before 1750 than it is during the period 1750–1840, when the comparison of Greenwich observations with Hansen's tables commences. The results which I have cited rest on the hypothesis that during this period Hansen's tables represent the observations. In view of the fact that the tables are based on these observations, and that the few scattered comparisons which Hansen has given do not show large systematic deviations, we would seem to be justified in provisionally accepting this agreement. At the same time, with so enigmatical a problem before us, everything must be the subject of critical inquiry; and I deem it quite possible that from time to time the tables may be found to deviate from observations about the beginning of the nineteenth century to an extent greater than has been supposed.

Feeling the importance of the comparison in question, the writer, some twenty years ago, commenced and carried nearly to an end a work which he was obliged to suspend without bringing it to a definitive conclusion. It was that of comparing Hansen's tables with observations of occultations of stars since 1750. Occultations of stars by the Moon were taken as the basis for the work, for the reason that the results are less subject to systematic error, especially the personal error arising from comparing the limb of the Moon with a star, than are meridian observations. But the results are still subject to a not inconsiderable accidental error arising from the inequalities of the lunar surface. It is therefore desirable to supplement them by the Greenwich Meridian Observations. These, up to 1830, were reduced by Airy, and the results published in his monumental work fifty-five years ago. In a subsequent work the reductions were carried forward to 1851. The results were compared with Damoiseau's tables, to which were applied a number of corrections.

It does not seem that any certain result can be derived from this comparison at the present time. After making all allowances for the possible errors of the corrected tables and the probable errors of observation, the outstanding differences are larger than

could be anticipated. Some years ago I endeavoured to investigate the subject by applying to the tables used by Airy the principal corrections necessary to reduce them to those of Hansen. But these corrections were too small to materially diminish the discrepancies. The case is rendered more embarrassing by the fact that when we take the few comparisons given by Hansen,* and compare them individually with those made by Airy, using the same observations, we can trace only the slightest relationship between the two.

As an example of this, I have made the following comparison of the two results at certain dates when the Moon was near the one or the other solstice, so that the residuals in longitude and latitude used by Airy should nearly correspond to residuals in R.A. and Dec. which are given by Hansen. Both sets are given in the sense Theory *minus* Obs.

		Δ Long.		Δ Lat.	
		Airy.	Hansen.	Airy.	Hansen.
		"	"	"	"
1751 Oct.	24	+ 2·1	+ 1·4	− 3·1	− 1·5
	25	− 0·3	+ 3·5	− 6·5	− 5·2
Nov.	7	+ 15·2	+ 1·6	− 8·3	− 7·0
1752 Feb.	23	+ 3·8	− 7·5	+ 2·2	+ 2·5
	25	+ 15·4	+ 0·2	+ 6·0	+ 3·9
Mar.	6	− 3·4	− 6·6	+ 5·0	+ 6·1
	7	+ 0·2	− 3·7	+ 5·3	+ 2·8
	9	+ 7·2	+ 0·7	+ 10·3	+ 3·7
Oct.	12	− 7·9	− 3·5	+ 12·7	+ 11·2
	13	− 8·1	− 2·7	− 5·2	− 6·0
	25	+ 3·1	− 0·2	− 2·5	− 7·7
	27	+ 1·7	+ 5·3	− 3·6	− 4·4
1824 Mar.	8	+ 7·7	0·0	− 0·2	+ 1·1
	9	+ 0·6	− 2·9	+ 0·3	− 0·6
Aug.	31	+ 8·0	+ 4·7	+ 6·3	+ 0·8
Sept.	1	− 1·3	+ 0·5	+ 2·8	− 2·5
	2	− 4·0	+ 0·7	+ 8·1	− 0·6
	15	+ 4·7	+ 0·7	+ 0·6	+ 0·4

When studying this subject twenty years ago I satisfied myself that these discrepancies could not be materially diminished by applying to Airy's results the differences between Damoiseau's theory, as corrected by Airy, and that of Hansen.

The investigation of the subject is rendered more difficult by the fact that no details of Hansen's comparison exist. The

* *Monthly Notices R.A.S.*, xv. pp. 2–7.

differences between theory and observation are all he gives. We find no statement as to methods or data of reduction, or the actually observed or computed positions of the Moon. Some twenty years ago, during a visit to Leipzig, I made an unavailing attempt to find the material in question among Hansen's papers, which are there preserved. A more thorough search might, however, bring something to light.

Whatever might be the outcome of such an investigation, it is very clear that Airy's reductions need a thorough revision, and that the results should be compared directly with Hansen's tables, the latter being corrected for their principal known errors. The great improvement made during the last half-century in our knowledge of the positions of the fixed stars, and in the data and methods of reduction generally, forms a sufficient reason for the suggested revision.

IV.

The question of correcting Hansen's tables before deriving any results from them is an important one. They undoubtedly need a number of corrections. Those first in importance, if we could only decide upon them, would comprise terms of long period in the mean longitude. But the nature of these terms is one of the very matters to be investigated, and it is therefore useless to make purely theoretical corrections that do not represent observations.

Among the corrections of short period, the most important is that required by Hansen's parallactic inequality. This is about $2''$ too large in his tables. The existence of such an error is difficult to account for if the inequality in question was determined by a direct comparison of the theory with observations. A curious fact in this connection is that if the value which Hansen uses as applicable to his disturbed mean anomaly had been used as an inequality in the true longitude, the result would have been nearly correct. The question whether Hansen took the one coefficient as equivalent to the other is not now of practical importance. What is of practical importance is to correct the tables for the error, which is more troublesome than any other in the comparison with observations. I did not do this in my former "Researches," because the amount of the correction was still uncertain; and it is easier to make a correction like this, once for all, through a long period than it is to proceed by successive patching up.

I believe that all the other corrections to the tables are, individually, only fractions of a second, and will, in their total effect, rarely, if ever, rise to $2''$. This total will also be a rapidly oscillating quantity, as often positive as negative. It can therefore not affect our conclusions as to the terms of long period.

V.

The importance of the questions suggested in the preceding discussion is emphasised by the growing need of new tables of the Moon. During the half-century which has elapsed since the completion of Hansen's tables great progress has been made in determining the relations and numerical values of the various elements on which such tables must be based. The great stumbling-block in the way of a perfectly satisfactory basis is the enigma of the variations of long period. The solution of this enigma requires two processes. One is the thorough investigation of the mathematical theory of the subject, the other the discussion and comparison of observations.

In considering the mathematical side of the work the first question is that of method. Thirty years ago I proposed that the problem should be attacked in the most rigorous way by regarding the twelve elements of the Sun and Moon as simultaneously variable in the general Lagrangian equations of motion. An attempt by this method is found in a memoir on the action of the planets on the Moon, of which the work was actually done in 1871–72, though the memoir was not published until 1894. I am now convinced that this method requires developments too complex to practically be carried through. The developments actually found in it can, however, to a large extent, be used in a modification which leads to what is, in effect, a continuation of Delaunay's method, and is practically identical with the method adopted by Hill and Radau. What is wanted reduces itself to a rigorous reconstruction of the fundamental equations of this method.

On the observational side the question may arise whether it is not better to await the completion of an improved theory of the Sun's action on the Moon—for example, that which Professor E. W. Brown is now bringing to a conclusion—before attempting a correction of the lunar elements by observations. So far as the terms of long period are concerned it is clear to me that this question must be answered in the negative. A mapping out of these deviations, as observed through a period of 250 years, may afford valuable suggestions as to where we must look for their cause. Moreover, the results that may be reached will be practically as good when Hansen's tables are used, with a few preliminary corrections, as if we had the most accurate tables possible.

The case is not so strong for going on to the immediate determination of the lunar elements by taking Hansen's tables as the basis of comparison. If printed tables, based on Brown's theory, were available, it would be better to use them. Yet the advantage thus gained would not be so great as to justify a very long delay in order to realise it. The theoretical errors of Hansen's tables, some easily corrected faults aside, are so small

that they would be completely eliminated in the mean of so long a series of observations as is now available.

More serious is the fact that the eccentricity and inclination which we should derive by the suggested plan would be those which belong to Hansen's system of variables, and not to the new theory. But it is so simple a matter to reduce one of these sets of elements to the other that this does not constitute a strong objection.

The following seems to me the logical order of proceeding as the case now stands :

1. A new investigation of the possible inequalities due to the action of the planets.

2. The completion of my comparison of Hansen's tables with occultations, in order to determine the actual deviations of long period.

3. The construction of provisional tables based on Brown's theory.

4. The re-reduction of the Greenwich Meridian observations since 1750.

5. The comparison of the results of this re-reduction with the tables of Brown or of Hansen.

The works here suggested involve such an amount of labour that a system of international co-operation in their prosecution seems desirable. The question whether my estimate of their importance is correct, and, if so, whether such co-operation is necessary and practicable, is one which I should be glad to see discussed.

Errata in Annual Report, Monthly Notices, vol. lxiii. No. 4 (Obituary of Col. Cooper).

Page 197, line 14 from bottom, *for* 1872 *read* 1863.

„ line 4 from bottom, *for* 1876 *read* 1874.